DISCUSSION ON TOPIC 24

"ESTIMATION FROM ACCELERATED TESTS AND
SHORT-TERM TESTS IN RELIABILITY AND MEDICINE"
(PRESENTED AT THE 46-TH SESSION OF THE
INTERNATIONAL STATISTICAL INSTITUTE)

. . . by

Nozer D. Singpurwalla

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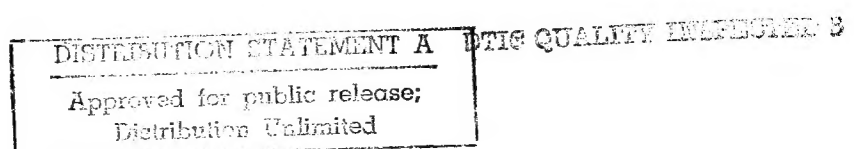Institute for Reliability and Risk Analysis

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Abstract

This is an invited discussion on three papers entitled "Parametric Models and Inference Procedures for Accelerated Life Tests" by G. K. Bhattacharyya, "Bayesian Inference in Accelerated Life Testing" by R. Viertl, and "Short-Term Tests for Genetic Toxicity: The Statistical Imperative" by B. H. Margolin, presented at the 46-th Session of the International Statistical Institute Meeting in Tokyo, Japan, 8-16 September 1986. The papers and the discussions are to appear in the *Bulletin of the International Statistical Institute* (Vol. 52).

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If the merit of a paper is to be judged on how much new information it offers, to a large proportion of its intended readership, then it is my position that all the three papers presented under Topic 24 be declared meritorious. However, all the three papers also suffer from a common drawback, namely, their failure to incorporated the Soviet work on the topic of the session. Whereas I am not cognizant the Soviet's efforts on Short-term tests for Genetic Toxicity, and I would be surprised if there is none, I am indeed aware of the important work done by the Soviet's on accelerated life testing (ALT) — see for example, Rukhin and Hsieh (1987) and the discussion by Singpurwalla (1987).

Notwithstanding the above limitation, Professor Bhattacharyya's paper is thorough, concise and covers a wealth of information; in paragraph 2 of Section 1, it presents a good survey of the modern practical applications of ALT with the one pertaining to prosthetic heart values as being quite appealing. Also appealing, and perhaps novel, is his idea of modelling the stochastic growth of fatigue crack via the inverse Gaussian distribution and allowing the rate parameter of the damage growth to be a function of the stress. Would this author care to comment as to whether the above approach, with its inherent sophistication, has produced

results in ALT which are in any way superior to those produced via the material scientist's Miner's Rule, upon which the Birnbaum-Saunder's fatigue life distribution is based?

Professor Bhattacharyya mentions that censored sampling is a way of reducing test time and that ALT and censoring are coupled in the same experiment with the common goal of saving on cost and time. I must take issue with this point of view. Whereas ALT provides information (albeit, some) about the item's behavior under use conditions stress, censoring results in *loss of information*; see for example, Brooks (1982) and Hollander, Proschan and Sconing (1985). The issue of information in ALT prompts me to suggest that one ought to undertake an investigation on the amount of information that is lost as one deviates more and more from the use conditions stress towards larger and larger values of the accelerated stresses. Such an investigation will be germane to the much neglected topic of experimental design in ALT. Also, Barlow's (1982) proposal that *expected entropy* be used as a measure of the information provided by an ALT, appears to be useful for undertaking the proposed investigation.

Other points in Professor Bhattacharyya's paper that need comment pertain to the LL relationship to stress, via the Arrhenius, the Eyring and the Inverse Power Law models. I am (now) of the opinion that the above Laws do not provide a scientific basis for relating the scale parameter θ of the failure model to the stress, with the shape parameter η being invariant (even though it was I, that had originally postulated such simplifying relationships). Some empirical work by Barlow, Toland and Freeman (1987) casts doubt on the above simplification making all the work on parametric life models with LL acceleration meaningful only for exponential life lengths. Under non-exponentiality, what seems more plausible is a

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relationship between the mean or the median time to failure and the stress, via the Laws described above. Glaser's (1984) postulated linear relationship between $1/\eta$ and the stress appears to be chosen for analytical convenience, rather than a physical basis. A final point about Professor Bhattacharyya's paper that needs mention pertains to the fact that the *cumulative exposure model* attributed to Nelson (1980) should be attributed to Sedyakin (1966).

Professor Viertl's paper focuses on Bayesian methods in ALT and raises some interesting issues, the two which attract my attention are his suggestion of using the *infinitesimal characteristic* to study the interrelation between the physics of failure mechanisms and lifetime distributions, and his suggestion of using *fuzziness* in ALT. Whereas I am fully supportive of the first (a use of this idea in ALT is due to Professor Viertl), I must take issue with the second, because unlike the calculus of probability, the rules of combination using fuzzy logic do not have an axiomatic justification, and would therefore lead to incoherent inference [Lindley (1987)].

The notion of *semi-sufficiency*, described in Professor Viertl's paper is intriguing. It's purpose is to assist Bayesian computations when the underlying failure model is a member of the *semi-exponential* family of distributions. With regard to this, two questions come to my mind, the first pertains to the relationship between semi-sufficiency and *B-sufficiency* (partially sufficient in a restricted Bayes sense) introduced by Basu (1977), and the second pertains to its usefulness. Note that semi-sufficiency results in a conjugate family of prior distributions for the scale and shape parameters of the Weibull distribution, and this happens to be the product of two gammas; thus the two parameters are a priori independent - this is not realistic. So what has been gained by semi-sufficiency other than a

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simplifying but questionable assumption?

Two final points about Professor Viertl's paper that need mention pertain to the fact that the Bayesian approach by Proschan and Singpurwalla (1979) *violates the likelihood principle,* since it pools the adjacent violators (as is also done in isotonic regression) and is therefore not Bayesian!. Also, the Kalman Filter approach of Blackwell and Singpurwalla (1987) overcomes some limitations of the Kalman Filter approach proposed by Meinhold and Singpurwalla (1984), and is therefore to be advocated for use in practice.

Dr. Margolin's paper captures the spirit of what good interaction between statisticians and scientists can result in. His discussion about some scientific issues in oncology and carcinogenicity should be educational to most statisticians; it certainly was to me. However, upon reading his paper and some of its references, I get two impressions: first, that in a statistical analysis of in vitro SCE data, and in the prediction of chemical carcinogenesis, the underlying models used for testing for increasing dose response, and the carcinogenicity outcomes, respectively, are based on statistical convenience rather than on biological principles. The analogy with ALT is choosing a time transformation function for its convenience. Second, there appears to be a <u>distinct</u> <u>absence</u> of the use of Bayesian methods in the biostatistical literature. One would hope that with the tremendous amount of background information that is now available in the biological sciences, the desire to formally incorporate it in biostatistical analyses would be strong; however, this is not the case - see for example Groer and Barlow (1982), Meinhold and Singpurwalla (1987), and Groer and Periera (1978). With respect to this last point, it appears that statisticians in the biological sciences are lagging behind their counterparts in the physical and engineering sciences with respect to a use of modern statistical

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technology. If Dr. Margolin agrees with this observation, would he care to comment

on the reasons for the above?

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